

RESPONSE TO NOAA RESEARCH CONCIL REVIEW

Review of CCSP Draft SAP 2.3

Aerosol Properties and their Impacts on Climate

Background

The Research Council, as part of its major project oversight duties for the Climate Change Science Program (CCSP), was asked to review the draft Prospectus for CCSP Synthesis and Assessment Product 2.3: Aerosol Properties and their Impacts on Climate.

The Research Council Exec Sec sent the document to all Research Council members and to specific NOAA staff that were identified to review the document. Following are the consolidated Research Council comments.

Council Comments

Executive Summary, paragraph 5: This paragraph should be revisited. The phrase "the ability of climate models to reproduce the global mean temperature change over the past 100 years appears to be the result of using a 'tuned' aerosol forcing," may not be accurate. Specific comments follow.

Comment 1 RE: Tuning: If it were not for the aerosol uncertainty, then the 20th century would provide a very good constraint on climate sensitivity. (Ocean heat uptake is also uncertain, but it is also constrained by other data). Given the aerosol uncertainty, we can generate models that fit the observed global mean temperatures evolution with differing aerosol forcings by compensating with differing sensitivities (ie, differing cloud feedbacks). (Conversely, if we had a better handle on cloud feedbacks, the observed trends would probably give us a good handle on aerosol forcing over the 20th century.) I think this is the most fundamental attribution problem that we face, and is, in particular, why modeling aerosol and aerosol-cloud interactions are the main focus of our atmospheric model development for AR5. The uncertainty seems mostly to be pushing towards the high side of climate sensitivity (and large aerosol forcing), by the way, rather than the reverse, as compared to the average AR4 model, so I would not look for this to motivate models with sensitivities lower than, say, 2K per doubling of CO₂.

At GFDL, we did not "tune" our aerosol forcing for our AR4 model in any way -- the climate model was finalized before we generated our 20th century simulations, using aerosols produced by a separate atmospheric chemistry model. NCAR also did not do any tuning as I understand their development path. Both of these models included only the direct effect of aerosols on radiation and did not attempt to simulate any indirect aerosol/cloud interactions. (We are currently struggling with precisely this question, however, as we begin to include indirect effects with large uncertainties in our AR5 model development.) GISS, on the other hand, did do something that one could call tuning. They did try to include some aerosol indirect effects, and, as I understand it, modified a parameter in their indirect effects scheme to produce the radiative forcing that they

estimated would be needed to simulate the 20th century trend. For the AR4 as a whole, several papers have noted that the simulations of the 20th century trends from the ensemble of models is somewhat better than you would expect given the range of cloud feedbacks and aerosol forcings, suggesting that there was selective pressure to work with combinations of sensitivity and aerosol forcing consistent with the observed temperature trends. There is nothing wrong with this in principle, as long as you make it clear that you are operating in this way so that others do not over-interpret the good agreement with the temperature record.

There is an implicit criticism of AR4 here -- not with any specific key attribution statements -- but the selection of figures in AR4 does have the effect of emphasizing the importance of the quantitative agreement between models and observed trends without, I think, giving this well-known aerosol/sensitivity tradeoff sufficient explicit prominence.

Comment 2 RE: Tuning: Paragraph states that the aerosol forcings in the AR4 models differ by a factor of 9! But there are a few rather strange models in this ensemble, and one can over-dramatize model differences in all sorts of ways if one doesn't eliminate outliers. It is generally better to talk about, say, the quartiles of the distribution (i.e. half the models are between x and y) to describe the model spread, especially in an executive summary.

Comment 3 RE: Tuning: The statement concerning 'tuning' of aerosols in the models employed by IPCC AR4 that reproduce the observed 20th century surface temperature evolution deserves a very close scrutiny. The suggestion is made, and in fact is more than an implication here, that the models' good simulation of the 20th century temperature evolution appears to be due to tuning of the aerosols. Care has to be taken to script such a statement. This statement is more likely incorrect as written.

First, the equilibrium climate sensitivity (computed for 2xCO₂ – 1xCO₂) may not be an accurate indicator of the transient climate sensitivity which is the more relevant parameter for this case. Second, it is not certain that the AEROCOM evaluation of the direct forcing (which the document refers to, and is possibly the only source of the information on forcings arising in the AR4 models) bears a 1-to-1 resemblance to the versions of the coupled models that were actually deployed to drive the transient climate evolution. In fact, it is likely that the range of forcings arising in the dozen or so coupled models just cannot be obtained. This was not possible simply because the coupled models whose temperature responses went into the attribution analyses had not calculated and/or documented their particular aerosol (or, for that matter, any species) forcing values for the transient runs (1860 to present, and to 2100). The lack of availability of aerosol forcing values from the coupled models does not mean that the aerosol forcing in them was 'tuned'.

It is also not clear whether quantitative conclusions concerning the AR4 models' aerosol forcings can be inferred unambiguously from the limited details supplied by the various modeling institutions. Note that the "forcing" assessment in AR4 (Chapter 2) is not necessarily the same as that for the models simulating the temperature response since the latter were never evaluated in terms of their forcing. While it is correct to point out that all the models more or less reproduced the temperature evolution remarkably well, and that this is still a puzzle which has not been sorted out, it needs to be also remarked that this

limitation is chiefly because such information was not asked for nor submitted by the participating models. The matching with the observations is not necessarily due to dubious implementations of aerosol physics in all the AR4 coupled models; it is hard to imagine that all models tuned it up. The circumstances leave an important question begging which may never be answered fully (since most institutions now have newer models and are preparing for the next IPCC), but this fact should not be used to suggest that there was some *ad hoc* tuning of the transient climate via tuning of aerosol forcing. While it would be correct to say that some degree of compensation may have occurred between the models' climate sensitivity and their aerosol forcings (direct + indirect) and to some extent differences in ocean heat uptake, it is not possible to assert this given that models did not document their respective aerosol (or any other species) forcing over the Industrial period. In a broader sense, how was the determination made about the quantitative range of the models' aerosol forcings, particularly since these values were never made known by the models performing the climate simulations? And, care has to be taken when introducing quantitative aspects about the 'indirect' effect since this consists of a suite of processes and not all of them can be characterized as 'forcing'.

Generally, models are tuned, but most tune the climate to present-day observations. Usually, clouds and convective parameterizations are toyed with to arrive at a reasonable present-day climate. After that, though, both the evolution of the climate and that of the future projections is not anchored or 'modified' (or 'tuned') in any way. I will list here the details of the operating procedure for the GFDL model used in AR4, to refute the assertion that models 'tuned' aerosol forcing. Here is what took place for the GFDL model. Emissions of aerosol precursors were prescribed on a decadal basis from 1860 to present, based on available international quantifications (which, in turn, is compiled using population estimates). For the future, the scenarios used in TAR and the values given there (see TAR Appendix) for the emissions were employed. These emissions were then fed into an offline chemistry-transport model (CTM) to obtain the monthly-mean spatial distributions. These distributions were then fed into the coupled climate model to simulate the transient climate response from 1860 to 2100. The CTM-simulated aerosol distributions were extensively compared against available observations – and there are clearly successes and failures as documented in the literature and in this report. The aerosol wet removal process, which are parameterized and have uncertainties about them, was 'tuned' to replicate present-day observations of aerosol concentrations (not forcing) as optimally as possible. This procedure was also applied to clouds for the present-day simulation. However, after these parameters were set to simulate present-day observations, they remained unchanged for both the past and the future integrations. At no time was the aerosol distribution or parameters simulated either in the past or in the future adjusted in any way. Thus, it is incorrect to say that the aerosol forcing was tuned, at least for this particular model. At best, only aerosol concentrations for present-day were, so-to-speak, tuned. Note that tuning of aerosol concentrations for present-day is not equivalent to tuning transient aerosol forcing or, more particularly, tuning aerosol-induced perturbation of radiative flux between some arbitrary period and present-day. And, tuning to present-day does not guarantee a good transient simulation throughout the 20th century. The GFDL model did not incorporate the complex suite of processes capturing the 'indirect' effect. The main reason for deciding not to implement it was the large uncertainty associated with the processes contributing to this effect. If the 'indirect' effect is small, this would not have

a bearing on the temperature evolution result that has been obtained in AR4. But, if it is large (say, upper end of the Chapter 2 estimate), then the model would not replicate the observed climate evolution well. Note though that the totality of the indirect forcing involves forcing as well as ‘feedback’ so that, with the incorporation of this effect, it may no longer be possible to analyze model responses as some distinct combined effects of ‘forcing’ and ‘feedbacks’.

What about the other models? What did they do about aerosols? It is fair to say that whether they included indirect effects or not, they most likely did not anything closely resembling to tuning aerosol forcing in a deliberate manner so as to obtain the transient observed temperature evolution.

In summary, unless there is compelling evidence in the literature to suggest ‘tuning’ of the temporal evolution of the aerosol forcing in several models, it would be unjustified to write such a statement in the SAP.

RESPONSE TO ALL COMMENTS ABOVE: The Executive Summary has been mostly re-written and the word “tuning” has completely removed. Instead, we have pointed out the that the aerosol forcing used in the IPCC AR4 models has a wide range of uncertainties that would compensate the climate model predicted GHG warming to different degrees to make the model predicted surface temperature change close to the observations.

Executive Summary, page iii, line 2: Is this Executive Summary intended to be accessible to non-technical audiences? If so, the level of specialized jargon and acronyms needs to be reduced considerably. A Reader's Guide should be included either as part of the Executive Summary or in a Preface (similar to that currently included in Section 1.4).

There is now a Glossary for technical terms. We have also made an effort to take out specific details in the Executive Summary and tone down the jargon.

Executive Summary, p.iii, l.14: Change "a suspension" to "suspensions".
Changed.

Executive Summary, p.iii, l.23, 26, 37: The terms "radiant energy", "radiative fluxes", and "spectral regions" are jargon and should either be avoided or at least defined before being used in the Executive Summary.

“fluxes” and “spectral” removed

Executive Summary, p.iii, l.31: Be clearer about which gases are being referred to here. Well-mixed greenhouse gases?

“gases” are now always identified as “greenhouse gases”

Executive Summary, p.iv, l.3-5: The rate of heat uptake by the oceans is another factor (besides different aerosol forcings) that could allow models with different climate sensitivities to all reproduce the observed historical temperature change.

Heat uptake by oceans is important, but seems to be outside of the focused discussion in the Executive Summary. We do not say or even imply that all the unknowns are in the aerosol

forcing, although we do say that the uncertainties in the aerosol forcing dominates the uncertainty in the total forcing.

Executive Summary, p.iv, I.4: Change "change, which" to "change that".

No longer applicable since the ES has significantly revised.

Executive Summary, p.iv, I.6-7, 11, 24-25: Avoid (or at least define) jargon terms such as "extensive aerosol properties", "intensive properties", "cloud microphysics", and "aerosol mass spectrometers" in the Executive Summary.

All these terms: "extensive aerosol properties", "intensive properties", "cloud microphysics", and "aerosol mass spectrometers" have been removed.

Executive Summary, p.v, I.1: Clarify how these sections of the Executive Summary map onto the report chapters.

The ES now follows the outline of the subsequent chapters.

Executive Summary, p.v, I.14: Define "secondary organic aerosols".

Secondary organic aerosols no longer mentioned in the ES.

Executive Summary, p.v, I.24-25: Define jargon terms such as "polarization", "phase function", and "cloud screening".

Jargons removed.

Executive Summary, p.v, I.43: Typo. "mulit" --> "multi".

Fixed.

Executive Summary, p.vii, I.20: Define "cloud-free aerosol DRE".

No longer used in ES.

Executive Summary, p.vii, I.20,32: The use of acronyms such as DRE and DCF throughout the Executive Summary makes this section less accessible for non-technical readers. Try to avoid extensive use of acronyms and jargon.

The acronyms used in the Executive Summary have been limited to IPCC AR4 (Fourth Assessment Report of the Intergovernmental Panel on Climate Change), AOD (aerosol optical depth), CCN (Cloud condensation nuclei), RF (radiative forcing), MODIS (MODerate resolution Imaging Spectroradiometer), MISR (Multi-angle Imaging SpectroRadiometer), CERES (Clouds and Earth's Radiant Energy System), CTM (Chemistry and Transport Model), and GCM (General Circulation Models, sometimes called Global Climate Models). Thus, 9 acronyms. Each is defined at the first use. We feel that this should not overburden the reader and actually helps in the readability of the Executive Summary.

Executive Summary, p.vii, I.32: Comment briefly here on how measurements can distinguish between anthropogenic and natural aerosols.

The last paragraph of section ES2.1 addresses estimating anthropogenic fraction from observations

Executive Summary, p.vii, I.42: This sentence is unclear. Rewrite, e.g. as "Aerosols scatter and

absorb different amounts of radiation depending on whether they are located above or below clouds. This scatter or absorption creates an aerosol effect"

Section has been revised.

Executive Summary, p.viii, I.42: Define "GCM", and try to avoid the use of jargon and acronyms in the Executive Summary.

GCM is defined.

Executive Summary, p.viii, I.43: Change to "for the climate change simulations in the IPCC Fourth Assessment Report".

Paragraph completely rewritten.

Executive Summary, p.x, I.28: Add "partially" after "the negative forcing by anthropogenic aerosols has".

Added.

Executive Summary, p.x, I.41-43: Sentence structure is unclear. Rewrite.

Completely rewritten.

NOTE regarding chapter 1 revision: This chapter has been almost completely rewritten in response to reviewer comments. Indeed, one of the Expert reviewers (Ralph Kahn) has served as lead author for the revised text. As such, essentially all the points raised have been addressed.

Chapter 1, p.1, I.2: Try to be more careful about defining technical terms before using them. Inset 3 is helpful in this regard, but should presume even less background knowledge.

Done. The definitions have been completely rewritten, focusing on accuracy and completeness, and Inset 3 was eliminated as too technical for the purposes of this report.

Chapter 1, p.1, I.30: Unclear sentence structure. Rewrite as "Earth's atmosphere consists primarily of a mixture of gases, but also contains particles such as aerosols and clouds."

This text has been entirely rewritten, so the suggested copy edit no longer applies.

Chapter 1, p.3, I.24: Change to "definition of *aerosol* forcing".

This text has been entirely rewritten, so the suggested copy edit no longer applies.

Chapter 1, p.4, Figure 1.2: This figure focuses exclusively on the shortwave forcing by aerosols. At least mention in the text that there is also a (smaller) longwave component to the forcing.

Done. Clear distinction between long and shortwave radiation budget components is now made, as appropriate, throughout the chapter.

Chapter 1, p.5, Fig. 1.3: The total aerosol forcing is given in this figure as -1.2 W/m² (range of -2.4 to -0.6). In Section 3.1.1, it is given as -1.2 W/m² (range of -2 to -0.6). Which of these is "correct"? Be consistent. Similarly, cloud albedo effect is given here as -0.7 W/m² (range -1.8 to -0.3). In Section 3.1.1, the range is given as -1.1 to +0.4.

Fixed. The text is now consistent, both internally, and with the IPCC AR4.

All the comments below (highlighted) regarding Chapter 1: This text has been entirely rewritten, so the suggested copy edits no longer apply.

Chapter 1, p.5, l.28: Add "(i.e., more negative)" after "much greater".

Chapter 1, p.7, l.10: Change "defined" to "known".

Chapter 1, p.7, l.27-30: Why do you switch from referring to "aerosol emissions" elsewhere to "PM emissions" in this paragraph?

Chapter 1, p.8, l.17, 19, 29, 37: Refer to the IPCC reports as the "IPCC First Assessment Report (1990)" and "IPCC Second Assessment Report (1995)", "IPCC Third Assessment Report (2001)", "IPCC Fourth Assessment Report (2007)".

Chapter 1, p.8, l.44-46: This sentence could be a bit misleading. While it is true that the modeling framework described does not allow the aerosol distribution to respond to climate change, the aerosol forcing is still a function of climate. That is, there is some interactivity (e.g., in water uptake). Also, the aerosol forcing is certainly a (strong) function of clouds in the climate model.

Chapter 1, p.9, l.14: Change "a" to "an". Also, if this chapter is intended to be "easily understandable", technical terms need to be defined before they are used (as noted in an earlier comment).

Chapter 1, p.9, l.23: Typo. "distributioins" --> "distributions".

Chapter 1, p.9, l.28: Change to "the runs described in the IPCC Fourth Assessment Report (2007)".

Chapter 1, p.10, l.20, 35: Table 1, referred to here, seems to be missing.

Chapter 1, p.10, l.29: Change "are" to "is".

Chapter 1, p.11, l.43: Explain what the refractive index is and how the real and imaginary components can be interpreted physically (e.g., in Inset 3).

Chapter 1, p.12, l.5-8: The notation used here differs from the earlier notation of DRE and DCF used in the Executive Summary.

Chapter 1, p.12, l.33: Change "scattered" to "scattering".

Chapter 2, p.18, l.3: Provide reference to and define "IPCC TAR".

Done.

Chapter 2, p.21, Figure 2.1: Figure caption is missing.
Fixed.

Chapter 2, p.34, l.26: Hyphenate "water-soluble".
Done.

Chapter 2, p.44, l.28: More standard spelling is "predominantly".
Corrected.

Chapter 2, p.48, l.19: Mineral dust doesn't cause "warming" at the TOA and surface. It causes a "positive forcing" (or perhaps a "warming tendency").
Corrected.

Chapter 2, p.51, l.7: Explain somewhere (e.g, Inset 3) the definition of the refractive index, and how the real and imaginary parts relate to physical effects such as scattering and absorption.
Refractive index is now explained in the newly added Glossary. Insets no longer exist.

Chapter 2, p.55, l.11: Change "10s" to "tens".
No longer applicable after this revision.

Chapter 3, p.75, l.41: Refer to the "IPCC Fourth Assessment Report (2007)".
ACCEPTED

Chapter 3, p.76, l.24, 27: As mentioned in Comment above for Chapter 1, the forcing estimates and ranges given here differ from those given in Figure 1.3.
ACCEPTED – MADE EQUAL

Chapter 3, p.77, l.31: Single quote missing after "extensive influences".
ACCEPTED

Chapter 3, p.78, Table 3.1: Indicate that the sulfate loadings, optical depths, and mass scattering efficiencies are for present-day (year 2000?) conditions, and that forcings and related quantities are for changes from pre-industrial to present-day conditions.
ACCEPTED

Chapter 3, p.79, l.8: Indicate (or reiterate) here which variables are "intensive".
ACCEPTED

Chapter 3, p.81, l.3: Change "1869-2000" to "1860-2000".
ACCEPTED

Chapter 3, p.81, Table 3.2: In column headings, add "POM" in columns 5-7, and "BC" in column 10.
ACCEPTED

Chapter 3, p.82, l.5-7: Here you say, "As was true for sulfate aerosols, the relative standard deviations of the intensive variables are as large or larger than those of the extensive variables" But, on p.79, l.5-6, you said that "the relative standard deviations of the intensive properties [for sulfate] were somewhat lower than those of the extensive variables".

ACCEPTED; CHANGED STATEMENTS TO BE EQUIVALENT

Chapter 3, p.83, l.27-28: Move this text (currently in square brackets) into the caption for Figure 3.3.

ACCEPTED [NOW FIG. 3.14]

Chapter 3, p.91, l.45: Period missing at the end of this sentence.

ACCEPTED

Chapter 3, p.92, l.22-24: Change to: "The GFDL model has been compared with observations by Ginoux et al. (2006). Results from that assessment are discussed in this section. A comparison between" The Ginoux et al. paper compares with fewer different satellite instruments than the Liu et al. paper, but it also compares the GFDL model extensively with AERONET and surface concentration measurement networks.

ACCEPTED

Chapter 3, p.92, l.27: Add reference to Horowitz (2006) after reference to Horowitz et al, 2003.

ACCEPTED

Chapter 3, p.92, l.27-28: Change to: "Dust concentrations were produced using a separate simulation with MOZART-2, with sources from Ginoux et al. (2001) and wind fields from the NCEP/NCAR reanalysis data."

ACCEPTED

Chapter 3, p.92, l.29: Add "Sea salt aerosols are not simulated directly with MOZART-2. Instead, monthly mean concentrations of sea salt are obtained from a previous study by Haywood et al. [1999]. They have assumed a concentration proportional to the wind speed and held constant from the surface to 850 hPa over ocean regions, with zero sea salt elsewhere."

ACCEPTED

Chapter 3, p.92, l.31: For sea salt optical properties, a constant relative humidity of 80% (loosely representative of the marine boundary layer) was assumed. Thus, the sea salt optical depth doesn't depend on the model's simulate relative humidity. (For sulfate, the model's relative humidity is used.)

ACCEPTED

Chapter 3, p.92, l.33-34: Delete "from which this comparison with observations is based."

ACCEPTED

Chapter 3, p.92, l.36: Mention that global emission totals of SO₂, BC, and OC, and burdens of sulfate, BC, and OC are given by Horowitz (2006).

ACCEPTED

Chapter 3, p.92, l.39-41: This sentence is unclear. Change to, e.g., "influenced by the growth of aerosols with relative humidity. In the GFDL model, sulfate is allowed to grow up to 100% relative humidity, but organic carbon does not increase in size as relative humidity increases."

ACCEPTED

Chapter 3, p.93, l.42-45: You should comment here on the comparisons of sulfate concentrations with surface observations. That is, can you comment on whether the excessive AOD is due to too much sulfate or too much extinction per unit sulfate.

ACCEPTED

Chapter 3, p.94, l.30: Summarize the major improvements in the representation of aerosols in the GFDL recommended by Ginoux et al. (2006): (1) better parameterization of hygroscopic growth at high RH, (2) better sea salt concentration distribution, (3) correcting an error in the OC extinction coefficients, (4) including hygroscopic growth of OC, and (5) improved biomass burning emissions inventory.

ACCEPTED

Chapter 3, p.92, l.32: The aerosol emissions, burdens, AODs, and forcings in the GISS and GFDL models (along with the NCAR model) are compared extensively in CCSP SAP 3.2, Chapter 3 (and also by Shindell et al., JGR, 2008). You should refer here to the comparisons done as part of that report.

ACCEPTED

The references for the CCSP report are (for the report as a whole and for Chapter 3):

CCSP, 2008: Climate Projections Based on Emissions Scenarios for Long-Lived and Short-Lived Radiatively Active Gases and Aerosols. A Report by the U.S. Climate Change Science Program and the Subcommittee on Global Change Research. H. Levy II, D.T. Shindell, A. Gilliland, M.D. Schwarzkopf, L.W. Horowitz, (eds.). Department of Commerce, NOAA's National Climatic Data Center, Washington, D.C., USA, 100 pp.

Shindell, D.T., H. Levy II, A. Gilliland, M.D. Schwarzkopf, L.W. Horowitz, 2008: "Climate Change From Short-Lived Emissions Due to Human Activities" in Climate Projections Based on Emissions Scenarios for Long-Lived and Short-Lived Radiatively Active Gases and Aerosols. H. Levy II, D.T. Shindell, A. Gilliland, M.D. Schwarzkopf, L.W. Horowitz, (eds.). A Report by the U.S. Climate Change Science Program and the Subcommittee on Global Change Research, Washington, D.C.

Chapter 3, p.94, l.37: What observations of "radiative impact" are you referring to? Suggest cutting "radiative impact" from this sentence.

ACCEPTED

Chapter 3, p.94, l.40: Note that the GFDL sulfate AOD exceeds that maximum value in Table 3.1 by a factor of 2.5.

ACCEPTED

Chapter 3, p.95, l.11: Change "Figs. 3.4" to "Fig. 3.4".

ACCEPTED

Chapter 3, p.95, l.29-32: Does the GISS model allow sulfate to continue to grow up to 100% RH? If so, perhaps the RH values in the GISS and GFDL models differ.

TAKEN INTO ACCOUNT: SEE DISCUSSION IN 3.2.3C

Chapter 3, p.95, l.32-35: Is sulfate allowed to deposit onto dust particles in this version of the GISS model? If so, what fraction of the sulfate is removed (for AOD and forcing purposes) in this manner?

NO IT IS NOT

Chapter 3, p.95, l.35-39: Comment further here on the similar total AOD in the two models despite very different contributions from individual species. See, for instance, CCSP SAP 3.2 Figure 3.2.

ACCEPTED

Chapter 3, p.95, l.43: Refer here to CCSP SAP 3.2, and give full citation (above).

ACCEPTED

Chapter 3, p.101, l.1: Which version of the GISS model is being referred to here as including both indirect effects? Is this the (or one of the) AR4 versions of the GISS model? Shindell et al. (JGR, 2007) reports that only the second indirect effect is included, and that its strength is empirically constrained (tuned) to give a desired amount of indirect forcing. This methodology needs to be discussed here more fully.

ACCEPTED; FURTHER DISCUSSION INCLUDED

Chapter 3, p.101, l.1-2: Unclear sentence structure. Rephrase, e.g., as "relating cloud cover to the aerosol number concentration, which in turn is a function of sulfate, nitrate, black carbon and organic carbon mass concentrations."

ACCEPTED

Chapter 3, p.101, l.8-13: Throughout this chapter, there needs to be a more clear explanation of what is included in all of the different versions of the GISS model(s) being presented. (For instance, the notes about different versions in Table 3.1 need to be explained.) Perhaps each version can be given a distinct name, such that the text can be clearer about which version is being used in each section of the chapter.

ACCEPTED; SEE BEGINNING OF 3.2.3A

Chapter 3, p.103, Table 3.4: Define "exp1", "exp2", etc. in Table footnotes. The current headings are not useful.

ACCEPTED

Chapter 3, p.108, l.20: Note that the GFDL model did not include any aerosol indirect effects.

ACCEPTED

Chapter 3, p.108, l.24: Did you intend the units of model sensitivity here to be "degC/(W m⁻²)"?

ACCEPTED. Sensitivity defined.

Chapter 3, p.110, l.4: Do you mean "Sections 3.2 and 3.3" here?

ACCEPTED; SECTIONS CHANGED APPROPRIATELY.

Chapter 4, p.124, l.23: Delete second occurrence of "accurately" in this sentence.

ACCEPTED

Chapter 4, p.124, l.36: Change "observe" to "estimate" or "determine".

ACCEPTED

Chapter 4, p.125, l.12: Change to "are necessary *to* develop understanding of these *relations* and to test *the* representation".

ACCEPTED

Chapter 4, p.126, l.14: Change "size-distributed" to "size-resolved".

ACCEPTED

Chapter 4, p.126, l.16: Typo, "rind down" should be "ring down".

ACCEPTED

Chapter 4, p.128, l.29-45: It is unclear what is meant here by "integration" of observations into global models. Are you referring to iterative improvement of models by comparison with observations, or to inverse modeling and/or data assimilation? While the latter are useful methods, it is less clear how they contribute directly to improved prediction/projection ability in the models.

ACCEPTED AND FURTHER EXPLAINED

Chapter 4, p.129, l.33-35: This sentence structure is unclear. Rewrite to clarify the intended meaning.

ACCEPTED

Chapter 4, p.130, l.3-6: You should mention here that SO₂ is, of course, another important aerosol precursor gas emitted to the atmosphere.

ACCEPTED